

sea of pumice which resulted from the eruption of Krakatoa, that you may perhaps not be indisposed to print it in NATURE.

University of Edinburgh, July 17

WM. TURNER

71, Rue de Genève, Aix-les-Bains, June 24, 1884

DEAR PROFESSOR TURNER,—I have been so occupied since meeting you in Edinburgh that it has been impossible to send sooner notes of my trip to Java. The first intimation that I had of anything unusual having occurred was on our way out to Australia. Making our way north again after having done our "easting" in about 45° S., we were all much struck by the splendidly vivid sunsets, and by the distinct interval of time between the actual disappearance of the sun below the horizon and the appearance of the deep red or crimson glow. This phenomenon was more striking as we sailed north. On reaching Queensland we heard of the volcanic eruption in the Sunda Straits and received the explanation of the so-called "Krakatoa Sunsets." I left Newcastle, N.S.W., for Batavia in a steamer about November 10, 1883, *via* Cape Leeuwin, sailing up the west coast of Australia. We first came across distinct evidences of the eruption about 200 miles before we entered the Straits of Sunda, in small isolated pieces of pumice-stone, which became much more numerous and finally took the form of yellow patches constituted by morsels of pumice varying in size from a pea or even smaller up to a cocoa-nut, rarely larger. As we neared Krakatoa itself, these patches were certainly more numerous and larger in size, but still the actual amount of debris was small, much smaller than I had expected to find after the account I had heard from persons who had previously traversed the Straits. The yellow patches were few and far between, and composed chiefly of a coarse dust with here and there larger lumps amongst it. Krakatoa, formerly the most fertile of all the lovely isles which one passes, rose like a vast cinder, still smoking. Not a blade of grass or a leaf was to be seen, just a grayish seared-looking mass. A large portion of the island had disappeared, and I was told that over the sunken part 300-fathom soundings had been taken. Between the island and Batavia we passed a few more floating patches of pumice as above described. After about eight days at Batavia we steamed down the coast to different ports, Cheribon, Tegal, and Samarang, being on the whole about three weeks away from the time we left Batavia to our return there, which took place, so far as I can remember, on January 1. On nearing Batavia again we passed through large patches of pumice-stone, patches of several acres in extent, some of the lumps forming them being of large size, roughly speaking about as big as a cwt. sack of coals, and all sizes below that down to coarse dust. We anchored for the night just outside the port in clear water. Early in the morning one of the officers called me to look at an immense floe of pumice-stone that was bearing down upon the ship, and very soon we were entirely surrounded, and formed the centre of about, I should think, a square mile. Though covering a large surface, there was evidently no great depth of matter. One could pick it up by throwing a bucket or heavy pail into the mass, and a small steam launch easily made her way through it to our side. We left Batavia early in the morning, and passed through two or three such collections, all making their way in the same direction by the action of a current, as there was no wind, the sea being perfectly calm. When about thirty miles from Batavia, we met coming towards us an immense field, similar, except for its greater extent, to those already described. I could not tell you by any means exactly how large a surface it covered, but at one time could only just make out the edge we had entered it at with the naked eye, and could not see its termination in the opposite direction with the ship's glass, so that it was at least several miles in extent. Also the depth of the pumice-stone bed was very great, offering considerable resistance to the ship's progress, as shown by its diminished speed. An iron fire-bar thrown over the side rested on the surface of the mass, instead of sinking. Large trunks of trees were not floating in the water, but resting on the surface of the pumice. The passage of our vessel left a wake of only a few feet, which speedily closed in again, so that to see it at all I had to lean over the stern and look under it as it were. It seemed exactly as if we were steaming through dry land, the ship acting as a plough, turning up on each side of her a large mound of pumice, especially noticeable on looking over the bows. Our passage through this made no great noise—just a soft sort of crushing sound. The effect was very striking and queer. I only regret that I did not time our passage through this, the largest mass we met. We passed through one more but smaller field in the Straits of Sunda, and after that do not remember again

meeting with any even small patches of pumice-stone. I thought it curious meeting such immense quantities of the debris in the same place where, a month or five weeks earlier, only a few scanty, isolated patches existed. It was not due to a new eruption, so must be accounted for by the currents massing together a large number of scattered patches; or perhaps a certain amount had first sunk, and then, later on, had risen to the surface. I hope these short notes may be what you want; if I can give you any more information, I shall be delighted to do so.

With kindest regards,

Believe me yours sincerely,

STANLEY M. RENDALL

The Laws of Volume and Specific Heat

THE former, known as the "law of Avogadro," implies that any given volume at the same temperature and pressure must contain the same number of molecules. It includes the law of Charles, viz. equal expansibility for equal increments of heat; and the law of Boyle or Marriotte, that the volume of any gas must vary inversely as the pressure.

The other is that of Dulong and Pettit; and as the former necessitated equal volumes, so this latter implies constant heats for parallel conditions. But, finding that few elements approximated this law, it was an early device to double, treble, or quadruple the old atomic weights to secure a supposed uniformity; and thus the law found this expression, viz. that the specific heat of any solid element would prove to be a measure of its atomic quantity.

This, put in plausible fashion, will be the stock instruction of the superficial books for some time to come; but in the higher circles of chemical life it is being admitted more and more that a great change has come over the spirit of this dream. Departures from the normal 6.4 are no longer attributed to errors of observation, and that constant is replaced by a range of 5.5 to 6.9; while, to keep within this, M. Weber has proved that the doubled carbon equivalent must be tested at a range of temperature exceeding 1000° C. He has found that within the limits of -50° and 600° its heat value increases sevenfold! Well indeed may he say, "The idea that temperature can be overlooked must no longer be entertained;" also, "That the specific heats are not generally expressed by constant numbers; the physical condition of the elements influence their specific heats as much as their chemical nature."

These be great admissions from one of the highest authority, but they are as nothing compared with the new demands of physical chemistry. Mr. J. T. Sprague, an able and determined new chemist, has been the first in England to challenge attention to the recent researches of M. Berthelot, L. Troost, and others of the very highest chemical authority.

In a recent paper he admits that the new results "strike at the root of the most favourite chemical doctrines of the day, doctrines which are the foundation of the modern atomic weight and molecular theory, and consequently of the doctrines of atomicity, and the complicated molecular theories which have been based upon the supposed atomicity and specific bonds of different atoms."

The laws of Avogadro and Dulong and Pettit are offshoots of one principle, and one really implies the other. If true, it would follow that the atomic heat must be the same for all substances, or, if otherwise, the same quantity of heat would not produce equal expansions; also that the specific heats must be equal at all temperatures, or equal quantities of heat would act differently at different temperatures, or else it must vary equally for all gases, or they would expand unequally for equal quantities of heat.

Now it is a misfortune for these laws that none of these conditions subsist over wide areas. As a consequence of the two laws, an air thermometer should measure all temperatures by equal rates of expansion, and a given expansion should correspond to a fixed quantity of heat; such a thermometer should also read equally if filled with any other perfect gas.

In other words, these laws can only be true if the relation between the weight and volume of different gases be constant, and if the heat absorbed in producing a given change of volume is equal at all temperatures; that is, if the specific heat is constant.

These conditions are practically fulfilled by air, O, N, and H, between 0° and 200° C., so that the scale of temperature derived from the change of volume is the same as the scale derived from quantities of heat; but between 200° and 4500° there is a gradual growth of changed conditions which proves fatal to both laws,

and there is apparently an absorption of energy which does not appear either in the form of expansion or of sensible heat as temperature. At this high stage the specific heat of some of the simple gases has increased threefold, while some gases have a greater rate of expansion than others.

The same thing occurs with other simple gases, but at a much lower temperature, as, even within 0° and 200° , where dissociation cannot be entertained, chlorine and other halogens differ considerably from N or H, and at 1600° , if an air thermometer indicated 1600° for a given expansion, a chlorine one would register by expansion 2400° for an equal temperature, though with a much greater absorption of heat by the chlorine.

This difference is dependent on the fact that at 1600° the comparative density of chlorine has diminished one-third; or, in other words, that its volume, as compared with H, instead of being 1, has become 1.5; or, to put it in another way, that under these conditions, the specific heat of Cl is threefold that of H.

Quite apart from these extreme cases the specific heat is never a constant value; it takes more heat to raise a given weight of substance 1° at one temperature than another.

The specific heat increases with temperature, but differently for different substances:—

	0° to 100°	0° to 300°
Iron	= '1098	'1218
Platinum	= '0335	'0343
Mercury	= '0330	'0350

The differences here are both distinct and small, but Be (glucinium) increases twofold within a moderate range, and we have seen that between -50° and 600° carbon increases its specific heat sevenfold, or, as Mr. Sprague expresses it: "The heat relation of each substance is described by a particular curve; and the small differences observed in some cases are not errors, but actual differences of the several curves, and where there is approach to identity it is accidental, due to the temperature of observation being within a limit at which the curves are near their commencement, and have barely begun to separate."

However tempting or fashionable it may be to rush into hypothetical explanations of half-digested truths, yet I have taken some pains to keep within facts, which are in some respects incipient and but little understood.

If the causal differences in the production of light and sound had been fairly or patiently entertained, the "luminiferous ether" would never have been invented, which now crosses our path, as an "opaque fact, stopping the progress of further knowledge."

If a little more humility and patience had been evinced in respect of the expanding facts connected with gaseous volumes and specific heats, the old equivalents would never have been doubled, trebled, or quadrupled, to mar the symmetry of a beautiful science.

I quite agree with M. Troost, who, in repudiating the hasty references to dissociation, &c., observes: "The only consequences which necessarily flow from the experiments at high temperatures, or at low pressures, are that the coefficient of expansion is variable with the temperature, or that the coefficient of compressibility varies with the pressure." Also with the final conclusion of M. Berthelot: "The only law absolutely and universally applicable to the elements is the invariability of the relations of weight according to which they combine. This notion, and that of the energy brought into play in their reactions, are the sole and only firm foundations of chemical science."

SAMUEL E. PHILLIPS

A Carnivorous Plant

WITH reference to Prof. Moseley's letter in your issue of May 22 (p. 81) on "A Carnivorous Plant preying on Vertebrata," I may mention that in 1881, when surveying at the Paracel Islands in the South China Sea, I saw a somewhat similar occurrence. The tide was low on the reef on which I was strolling and admiring the lovely forms of coral existence. As I neared a pool cut off by the tide from the sea, I noticed amongst other submarine verdure a very ordinary-looking flesh-coloured weed about one foot high and of similar girth. My appearance alarmed numbers of tiny fish, which darted to the cover of overhanging ledges, but I noticed about half a dozen apparently seeking cover in the weed. Bending down closer, I saw that they were lying helpless about the fronds, with very little life left in them. Putting my hand down to pick up one of the half-dead fish, I found my fingers sucked by the weed, the fronds of which

closed slightly on them. The fish were not caught by the head especially, but held anywhere round the body. The death seemed to be slow and lingering, and where the fish had been held its skin was macerated. These captives may have been caught some time, and were in different stages of exhaustion. I regret being unable to name the plant, or the young fish. They were from an inch to an inch and a half long. The plant had a dirty and rather slimy look about it. ALFRED CARPENTER

H.M.S. *Myrmidon*, Suakim, Red Sea, June 24

Phosphorescence of the Jelly-Fish

THE conclusions arrived at by Mr. Verrill (NATURE, July 17, p. 281) cannot fail to be of interest to all who have ever speculated on the significance of the luminosity displayed by so many *Acalephæ*, *Medusæ*, and other marine organisms. When in the tropics, in 1875, very similar ideas occurred to me, and in an address on the phenomena of cyclical propagation delivered to the Essex Field Club on January 28, 1882, I ventured to put forward the following views, which, as the address is still in manuscript, I will beg permission to quote:—"It was in the Bay of Bengal, when on the Eclipse Expedition of 1875, that I first saw shoals of *Medusæ* in their full splendour. Speculating on the meaning of the vivid colours and brilliant phosphorescence of these creatures, I came to the conclusion that both these characters might be protective danger-signals of the same nature and fulfilling the same function as the bright colours of distasteful caterpillars according to Wallace's well-known theory, or the phosphorescence of the *Lampyridae* according to Thomas Belt ('Naturalist in Nicaragua,' p. 320). The 'urticating' powers of the jelly-fish would certainly make them unpleasant, if not absolutely dangerous, to predatory fish, and their bright colours and luminosity at night may thus be true warning characters."

R. MELDOLA

London, July 21

Fireball

RECORDED personal observations, such as that of Miss Annie E. Cocking (NATURE, p. 269) last week, must needs be so rare that every detail of them—especially where the description is clear and simple—is of weight and value. What strikes my own mind as of much interest in this one is that, as the strange and fateful visitant sank towards the carpet, "at this instant a peal of thunder crashed over the house—it was the very loudest the writer had ever heard." This would seem to show that, whatever the nature of the insulator which envelopes these floating Leyden jars, their connection is maintained unbroken with the cloud of origin until the moment of discharge; and that, whatever causes the "crash," a peal of thunder takes effect rather in the cloud than at the point of contact. This agrees also with the descent of a fireball in the sea at Margate, mentioned in to-day's papers, where the crash of thunder occurred while the ball was yet in sight. But it is still another question whether these floating globes, which only discharge themselves on contact, do not in some important respect differ in their nature from the commoner "fireball" discharged with the directness, if not all the speed, of a lightning flash out of a thundercloud. It is a question towards the solution of which only observations such as that for which we are indebted to Miss Cocking can materially help us.

HENRY CECIL

Bregner, Bournemouth, July 21

Animal Intelligence

THE following instance of animal intelligence may interest some of your readers. While walking through the forest here the other day, I found a young jay upon the ground scarcely able to fly. As I stooped down to examine it I was somewhat startled by a swoop made at my head by the old birds, their wings actually touching my hat. Determined not to be driven away, I remained by the young bird, whereupon a succession of like swoops were made at my head; these I easily succeeded in parrying with my stick, although the old birds frequently came in different directions. After about a couple of minutes the old birds seem to have come to the conclusion that nothing could be achieved in this fashion, and one of them, flying to some little distance, kept calling to the younger one, who half hopped, half flew after her. I of course followed; and now occurred what seems to me a striking instance of animal sagacity. The pines here are covered with lichen and a long, hairy kind of moss,